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Effect of at-birth cash transfers and payment design on child hospital presentations and maltreatment: a quasi-experimental study in South Australia

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Summary

Background. Cash transfers around birth may improve child health by reducing financial stress and helping families meet urgent household and infant needs. However, causal evidence remains limited on whether payment design affects health impacts, whether unconditional payments influence child safety, and whether benefits extend to older siblings. We examined the effects of an unconditional cash transfer at birth on child health and safety, and whether changing the transfer from a lump sum to instalments altered these effects.

Methods. We used linked administrative data for all births in South Australia to evaluate two reforms to Australia's Baby Bonus program: the 2004 introduction of a lump-sum payment of A$3000 and the 2009 change from a lump-sum payment of A$5000 to 13 fortnightly instalments. We estimated difference-in-discontinuities models comparing outcome discontinuities at each reform date with discontinuities at the same date one year earlier. Primary outcomes were any hospital presentation or admission and any Child Protection Services notification for the focal infant in the first six months after birth. We also examined older sibling outcomes, later follow-up windows, diagnosis-specific hospital outcomes, and maltreatment subtypes.

Findings. The 2004 lump-sum payment reduced infant hospital presentations and admissions by 9·8 percentage points (95% CI 7·3 to 12·2), from a pre-reform rate of 22·1%. Reductions were concentrated in respiratory and digestive conditions. Older siblings also had fewer hospital presentations and admissions. The 2009 change to instalments increased infant hospital presentations and admissions by 2·8 percentage points (95% CI 0·1 to 5·5), from a pre-reform rate of 18·4%. Neither reform was associated with detectable changes in Child Protection Services notifications.

Interpretation. At-birth cash transfers can improve infant and sibling health without increasing maltreatment risk, but payment design shapes effectiveness: a lump sum produced larger health benefits than the same amount delivered as instalments.

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Research in context

Evidence before this study

We searched PubMed and Google Scholar for studies published up to May 11, 2026, on birth-related or early-childhood cash transfers and child health or maltreatment outcomes. Search terms combined "baby bonus", "birth grant", "cash transfer", "child benefit", or "Earned Income Tax Credit" with "child health", "hospitalisation", "infant health", "child maltreatment", "child protection", or "child abuse". No language restrictions were applied. Existing causal evidence suggests that family income support can improve birth outcomes, infant hospital use, and longer-term child health and educational outcomes. However, evidence is limited on whether payment design affects health impacts, whether unconditional payments affect child maltreatment risk, and whether benefits extend to older siblings within recipient households.

Added value of this study

We used linked administrative data covering all births in South Australia and evaluated two reforms to Australia's Baby Bonus program: the 2004 introduction of an unconditional lump-sum payment at birth and the 2009 change from a lump sum to fortnightly instalments. Our difference-in-discontinuities design compares outcome discontinuities at each reform date with discontinuities at the same calendar date one year earlier, accounting for seasonal patterns in hospital use and child protection reporting. The lump-sum payment reduced infant hospital presentations and admissions, with reductions concentrated in respiratory and digestive conditions. Hospital presentations and admissions also fell among older siblings. The shift to fortnightly instalments increased infant hospital presentations and admissions, partially reversing the earlier health gain. Neither reform increased Child Protection Services notifications, including across maltreatment subtypes and follow-up windows up to two years.

Implications of all the available evidence

Cash support around birth can improve infant health without increasing child protection notifications. Our findings indicate that payment design matters: the same total payment can have different health effects depending on whether it is delivered as a lump sum or in instalments. Governments designing birth grants, child benefits, child tax credits, or other family income support should consider not only payment size and eligibility, but also disbursement structure. Payments around birth may be most effective when they allow families to meet large, urgent, or indivisible costs.

Introduction

Early infancy is a period of heightened vulnerability, when household resources can shape the conditions that protect or harm child health. Financial pressure around birth may affect housing quality and stability, food security, transport, exposure to household stress, and parents' capacity to meet urgent infant needs.[1,2] These pathways can influence hospital presentations and admissions, and may also affect risk factors that bring families into contact with child protection systems. For public health policy, key questions are whether cash support around birth improves child health and safety, which children and families benefit, and whether payment design shapes these effects.

Governments use a range of policies to support families around birth, including birth grants, child benefits, tax credits, paid parental leave, and other family payments. These policies are often debated in relation to fertility, poverty, or household income, but their public health relevance is broader. By changing the material and psychosocial conditions of early childhood, cash transfers may affect preventable illness, child safety, and health inequalities.

Existing evidence suggests that income support for families can improve birth outcomes, infant health, and longer-term child outcomes.[3-10] However, three important gaps remain. First, little is known about whether payment design matters. Lump-sum payments may allow families to meet large upfront expenses, reduce debt, or address urgent needs, whereas instalments may better support regular household costs. Second, causal evidence on whether unconditional cash payments affect child safety is limited, despite concerns that payments could increase harmful consumption or, conversely, reduce stress-related risks. Third, because payments are received by households, effects may extend beyond the newborn to older siblings, yet these spillovers are rarely measured.

We address these gaps using two reforms to Australia's Baby Bonus program: the 2004 introduction of an unconditional lump-sum payment at birth, and the 2009 change from lump-sum payment to fortnightly instalments over six months. Together, these reforms allow us to examine both the effect of introducing cash support at birth and the effect of changing how that support is delivered.

Using linked administrative data covering all births in South Australia, we exploit the sharp timing of these policy changes through a difference-in-discontinuities design. We estimate effects on hospital presentations and admissions and Child Protection Services notifications for infants and their older siblings during the first two years after birth. The findings provide causal evidence on whether birth-related cash transfers improve child health and safety, and whether payment design shapes their public health effects.

Methods

Policy setting and study design

On 1 July 2004, the Australian government introduced the Maternity Payment, colloquially known as the Baby Bonus. This payment was an unconditional, tax-free lump-sum payment of A$3,000 per newborn, available to families at all income levels, so long as claims were lodged within six months of birth. The policy was announced less than two months before implementation (on 11 May 2004), so any child conceived in response to the announcement would not be born within the first several months of the program's introduction.

Throughout the program's life, there were concerns that lump-sum payments were spent on non-essential items rather than on child-related costs.[11] These concerns motivated a reform that took effect on 1 January 2009, when the disbursement structure changed. Families with births before this date received A$5,000 as a single lump-sum (the amount was increased to this amount on 1 July 2008), while families with births on or after 1 January 2009 received the same cash amount, but paid in 13 fortnightly instalments. The stated rationale was to align payments with regular household bills and ensure funds supported ongoing child needs rather than being spent all at once.[12]

The 2009 reform also introduced an income test, limiting eligibility to families earning A$75,000 or less in the six months following birth. However, 94% of families remained eligible,[13] meaning the reform did not meaningfully restrict coverage. See appendix Table A1 for more program details.

Data Sources

Our data are from the iCAN (impacts of Child Abuse and Neglect) study which linked administrative records across government agencies that form a population-level de-identified dataset of all children born between 1986 and 2017 in South Australia. Linkage is at the person level and extended to families using mother-child links, to identify siblings from the same mother.

The study cohort was drawn from the South Australian Births Registry and the South Australian Perinatal Statistics Collection, which together provide exact birth dates and infant and maternal characteristics. Hospital records come from the South Australian public hospital administrative data and include all inpatient admissions and emergency department presentations, with month and year of contact and ICD-coded diagnosis (ICD-10-AM). Child protection records come from the South Australian Department for Child Protection and include all notifications of suspected maltreatment received by the agency, with information on the date of notification, investigation outcomes, and type of maltreatment.

Study Population

We exclude children deemed medically vulnerable, defined as those with a birthweight below 2,000 grams, a gestational age below 34 weeks, or born with a congenital medical condition.

These children, who represent approximately 5% of the base cohort, are excluded because their healthcare utilisation in the first months of life is driven primarily by clinical need, rather than parental resources or behaviour.

In addition, for the 2009 reform analysis only, we exclude children whose mothers were younger than 18 at the time of birth, representing approximately 1·5% of the sample. Mothers under 18 had already been moved to fortnightly instalments under a separate 2007 policy change, meaning the 2009 reform did not alter their payment structure.

Our final analysis samples ranged from 16,322 to 17,123 births for the 2004 reform and from 13,605 to 15,063 births for the 2009 reform, depending on the regression discontinuity bandwidth used. Descriptive statistics for the main outcomes and child and family characteristics are reported in Table 1.

Outcomes

Hospital utilisation is measured with a binary indicator for whether the child had any inpatient admission or emergency department presentation. ICD-10-AM codes allow disaggregation by clinical cause. The second main outcome is child maltreatment, measured with a binary indicator for whether the child received a Child Protection Services notification. A notification is a report made to authorities, often by a mandatory reporter (covering the majority of workers who engage with children), due to concerns that a child has been exposed to significant abuse or neglect.

Our primary analysis focuses on hospital utilisation and maltreatment in the first six months after birth, as this is the period during which most families would have received and spent the cash transfer and where we would expect the strongest response. We also examine outcomes in subsequent non-overlapping six-month windows (6–12, 12–18, and 18–24 months after birth) to assess whether effects persist, fade, or emerge later.

We construct the same two outcomes for older siblings aged under ten years at the time of the focal child's birth, measured over equivalent post-birth time windows. The mean age of siblings in our sample is 4 years.

Statistical Analysis

The standard approach for evaluating policies like the Baby Bonus is a regression discontinuity design, which compares outcomes for children born just before and just after the eligibility cutoff date.[7,8,14] However, a standard regression discontinuity design may be biased in our setting because outcomes could differ around each cutoff for reasons unrelated to the policy. This concern is particularly acute for the 2009 reform, where the cutoff falls on 1 January, a period when holiday staffing levels in hospitals differ from normal and families may interact with health services differently. We therefore extend the standard approach using a difference-in-discontinuities design, which compares the discontinuity in outcomes at the cutoff date in the reform year to the discontinuity at the same calendar date one year earlier, when no policy change occurred. This accounts for seasonal patterns that are common across years.

Another empirical issue is the potential manipulation of birth timing around each cutoff. Evidence suggests that birth timing manipulation occurred around the introduction of the Baby Bonus, but suggests it was mostly within one fortnight either side of the policy start date.[14,15] Exclusion of children born within 14 days either side of each reform date from the sample – a "donut" approach – is therefore used.

We implement the difference-in-discontinuities approach by estimating linear regressions with a covariate denoting program eligibility and a daily time trend term with a coefficient that differs on each side of the cutoff and in each year. The coefficient on the treatment indicator captures the difference-in-discontinuities estimate: the change in outcomes at the cutoff in the reform year relative to the same date in the prior year. The number of birth dates included on either side of the cutoff (the bandwidth) is chosen using an optimal bandwidth approach.[16] For the 2004 reform, this yields bandwidths of 103 days for CPS notifications and 107 days for hospital utilisation. For the 2009 reform, the corresponding bandwidths are 88 and 81 days.

We find no evidence of sorting around either cutoff and no meaningful differences in observable characteristics between children born just before and just after each reform date (appendix Figures A1 and A2, and Table A2), which indicates that babies born just before and after the reform are essentially randomised. We also demonstrate robustness to alternative bandwidth choices, donut widths, polynomial time-trend specifications, standard error clustering approaches, separate estimation for hospital admissions and emergency department visits, inclusion of mothers under 18 in the 2009 reform, alternative comparison years, extended outcome windows, count rather than binary outcomes, disaggregation by maltreatment type and severity, and inclusion of children deemed medically vulnerable at birth (appendix Figures A3 to A6 and appendix Tables A7 to A10).

Role of the funding source

The funder of the study had no role in study design, data collection, data analysis, data interpretation, or writing of the report. The corresponding author had full access to all the data in the study and had final responsibility for the decision to submit for publication.

Results

Effects of the 2004 Lump-sum Introduction

The introduction of the lump-sum payment had a large and statistically significant effect on hospital utilisation in the first six months after birth (Figure 1). Eligible infants were 9·8 percentage points (p<0·01) less likely to use a hospital service in this period, corresponding to a 44% reduction from a pre-reform rate of 22·1%.

Effects in later windows (6–12, 12–18, and 18–24 months) are small and statistically insignificant, suggesting the benefit is concentrated in the period when families received and likely spent the payment. We find no detectable effect on CPS notifications in any time window. Point estimates are small and not statistically distinguishable from zero, providing no indication that a lump-sum cash payment at birth increases harmful consumption and child maltreatment risk.

Effects of the 2009 Conversion to Fortnightly Instalments

The 2009 conversion from a lump-sum to fortnightly instalments produced a strikingly different result. For hospital utilisation, the reform is associated with an increase of 2·8 percentage points in the first six months (p<0·05), corresponding to a 15% increase from a pre-reform rate of 18·4% (i.e., the rate among 2008 births, who received the lump-sum). Compared with the pre-Baby Bonus rate of 22·1%, the 2009 reform reverses approximately 29% of the 2004 health gain, assuming that effect persisted through 2008.

As with the 2004 reform, the effect is limited to the first six months, with later windows showing small and statistically insignificant effects. We find no detectable effects on CPS notifications across any time window, consistent with the 2004 findings.

Effects by Hospital Diagnosis Category

For the 2004 reform, the largest effect occurs for respiratory conditions, with a 1·8 percentage point decrease (p<0·01) from a pre-reform rate of 4·6% (Figure 2). Digestive conditions show a smaller but still significant reduction of 0·9 percentage points (p<0·05), against a pre-reform rate of 2·0%.

For the 2009 reform, the pattern of effects is consistent with a reversal of the 2004 effects. None of the diagnosis-specific estimates for the 2009 reform reach statistical significance individually, but the point estimates are positive and substantial relative to baseline levels: a 0·6 percentage point increase in respiratory conditions and a 0·3 percentage point increase in digestive conditions.

We have similarly estimated effects on disaggregated maltreatment notification categories (neglect and physical, sexual, and emotional abuse). All estimates are small and statistically insignificant (see appendix Figure A7).

Effects on Older Siblings and by Birth Order

If the cash transfer were spent exclusively on goods and services specific to the newborn, such as cots and prams, we would not expect significant improvements in the health of older siblings in the household. We also would expect less of an improvement at the birth of a later-born sibling relative to firstborns, as large items for the newborn would likely be already acquired. However, we find that the 2004 lump-sum payment significantly reduced hospitalisations among older siblings. In the six months following the new baby's birth, hospitalisations decreased by 5·0 percentage points (p<0·05) from a pre-reform rate of 24·7% (Figure 3). This is smaller than the corresponding effect for the focal newborn, but suggests the payment generated broader household benefits. For the 2009 reform, point estimates for older siblings are 5·0 percentage points (p<0·1), mirroring the pattern seen for the newborn. We find no significant sibling effects on CPS notifications for either reform (appendix Table A5).

Comparing first-borns to later-born children (see Figure 4), for the 2004 reform, the reduction in hospitalisations is present for both first- and later-born children, with no meaningful difference in magnitude. This supports the analysis of older siblings, suggesting the payment was not spent exclusively on items for the baby. For the 2009 reform, the reversal of the health benefit becomes statistically significant among later-born births (3·5 percentage points, p<0·10), with a similar but insignificant pattern for first births.

Discussion

Our results demonstrate that how a cash transfer is delivered – not only whether it is delivered – shapes its impact on child health. The 2004 lump-sum payment is estimated to have reduced hospital utilisation by 44% in the first six months. This is larger than the 11% inpatient effect found in a prior study using a different method and a 12-month post-birth window.[7] Changes in hospitalisation are unlikely to reflect changes in ability to pay for healthcare given broad access to publicly-funded hospital and child healthcare services in Australia. Instead, they more likely reflect improvements in children's health. The significant reduction was concentrated in respiratory conditions, which are among the most common causes of preventable infant hospitalisation, and are influenced by household environmental factors, including crowding, heating, and exposure to tobacco smoke, all of which can be improved with additional financial resources.[17,18]

In contrast, converting the payment to fortnightly instalments was associated with a significant increase in hospital utilisation, reversing some of the health benefits associated with the lump-sum payment. Although we cannot directly observe how families spent the transfer, the pattern of results is consistent with lump-sum payments giving families the cash on hand needed to meet large or urgent expenses. This could include improving housing conditions, purchasing essential equipment, or paying down debts. This interpretation aligns with evidence from low- and middle-income country settings showing that lump-sum recipients are more likely to invest in durable goods and housing improvements.[19]

The finding that spreading payments reverses the health benefit is particularly striking given that the 2009 reform was explicitly motivated by concerns that families were spending the lump-sum inappropriately. Our results suggest the opposite: the lump-sum appears to have been spent in ways that meaningfully improved infant health, while the switch to fortnightly payments undermined these gains.

We find no evidence that cash transfers at birth increase child maltreatment risk. This null result holds across both reforms, all time windows up to two years, and all maltreatment types examined. A persistent concern in policy debates is that unconditional cash payments could increase harmful consumption, leading to higher rates of abuse or neglect.[20] Our findings do not support this concern. This result is consistent with recent US evidence that expanded cash transfers to families are not associated with increases in child maltreatment[21] and adds to the limited causal evidence base on cash transfers and child protection outcomes.[22]

Our sibling and birth order findings help us understand how families used the payment and suggest that evaluations focused solely on the index newborn may understate the full benefits of birth-related cash transfers. Under the 2004 reform, older siblings experienced a significant 5·0 percentage point reduction in hospital utilisation in the six months after the birth. The concentration of the sibling effect in the same time window as the newborn effect is consistent with a shared household resource story, in which the lump-sum cash transfer generated benefits for all children simultaneously rather than being directed solely at the newborn.

The findings by birth order reinforce this interpretation. If the payment were used primarily for newborn-specific purchases, we would expect effects to be larger for first births, where such items have not previously been acquired. However, we find no such pattern.

Taken together, these findings point toward general household expenditure as the primary channel through which the payment improved child health. This is consistent with survey evidence suggesting that Australian families used the Baby Bonus to increase spending on food and groceries and to reduce financial stress and hardship,[7] and with a growing literature showing that many families have limited cash on hand around the time of birth, even if they are not in persistent poverty.[1,2]

Our study has several limitations. First, our data come from one Australian state, and results may not generalise to other settings with different healthcare systems, child welfare frameworks, or social safety nets. Second, we cannot directly observe how families spent the Baby Bonus. Without this information, the precise pathways through which payments affected infant health remain unknown, and our evidence on mechanisms is necessarily indirect. Third, our child protection outcome captures CPS notifications (formal reports of suspected maltreatment) rather than actual abuse and neglect. True maltreatment, particularly lower-severity forms, frequently goes unreported and therefore unobserved in administrative data.

Our findings from Australia's Baby Bonus program show that birth-related cash payments can meaningfully improve child health without increasing child maltreatment risk. But program design also matters. A shift from a lump-sum payment to fortnightly instalments, introduced out of concern that families were using the lump-sum inappropriately, appears to have undermined a significant share of those health benefits. Policymakers considering baby bonuses or other forms of family income support should weigh this evidence carefully. The case for cash is strong, but so is the case for delivering it in a way that allows families to use it when and how they need it most.

Contributors

NB, DWJ, and TAT conceived and designed the study. EG and LS led the data linkage and curation. NB, DWJ, and TAT conducted the statistical analysis and interpreted the findings. NB, DWJ, and TAT drafted the manuscript. EG and LS reviewed the manuscript and approved the final version for submission. All authors had final responsibility for the decision to submit for publication and accept responsibility for all aspects of this work.

Declaration of interests

We declare no competing interests.

Data sharing

The de-identified linked administrative data used in this study were provided under data custodian agreements with the South Australian Department for Health and Wellbeing, the South Australian Department for Child Protection, and SA NT DataLink. These data cannot be shared by the authors because of the conditions of these agreements. The Stata analysis code used to produce the results reported in this manuscript is available from the corresponding author.

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Table 1: Pre-reform outcome rates and baseline characteristics of the 2004 and 2009 reform analysis samples

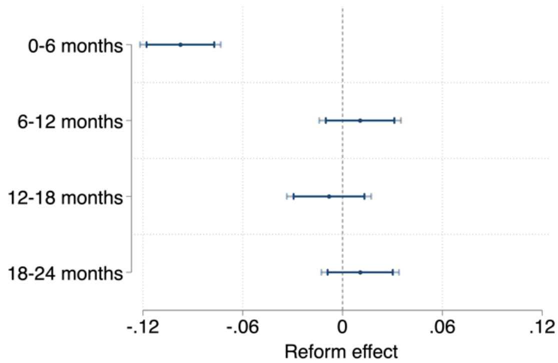
	2004 reform (Payment introduction)		2009 reform (Payment design changes)	
Characteristic	**Pre-reform**	**Post-reform**	**Pre-reform**	**Post-reform**
Sample size (N)				
Hospital utilisation analysis	8502	8621	6709	6896
CPS notifications analysis	8096	8226	7445	7617
Outcomes, first 6 months after birth				
Any hospital utilisation (%)	22·1	19·5	18·4	22·2
Any CPS notification (%)	2·2	2·6	3·5	3·7
Child/family characteristics				
Female (%)	49·2	48·6	49·4	48·6
First-born (%)	47·3	46·9	45·9	46·7
Maternal age at birth (years)	29·4	29·3	29·9	29·8
Mother unmarried (%)	12·6	12·5	10·7	10·6
Disadvantaged SES (%)	28·5	27·9	30·6	29·3

CPS=Child Protection Services. SES=socioeconomic status. Samples include births within the optimal bandwidth around each reform cutoff (1 July 2004 or 1 January 2009), with a 14-day donut excluded. Bandwidths: 107/103 days (hospital/CPS, 2004) and 81/88 days (hospital/CPS, 2009). Child and family characteristics use the hospital bandwidth. Disadvantaged SES is defined as belonging to the most disadvantaged quintile of the Index of Relative Socio-economic Disadvantage (IRSD).

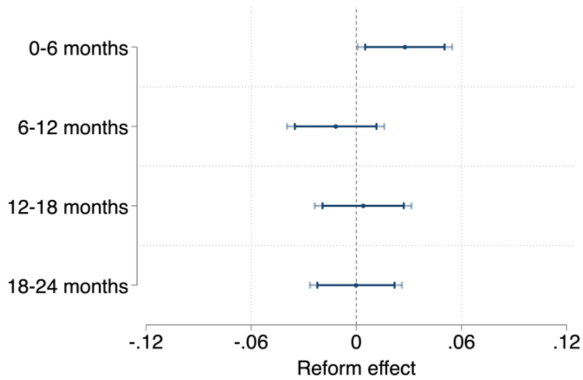
Figure 1: Effects of the introduction and changes to payment design of the Baby Bonus on hospital utilisation and safety

Any hospital utilisation

A) Effect of introduction of lump-sum payment

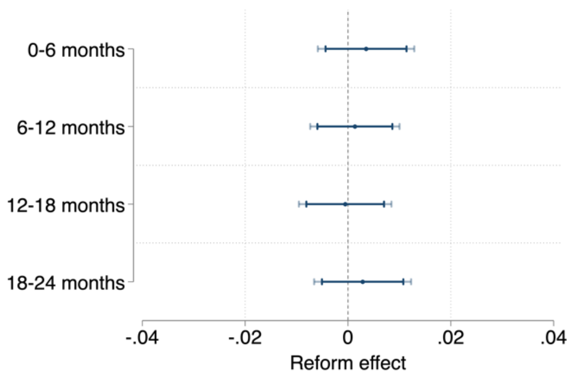


B) Effect of change to fortnightly payments

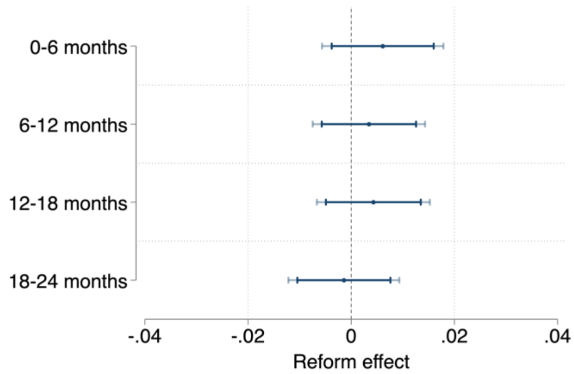


Any child protection services notification

C) Effect of introduction of lump-sum payment



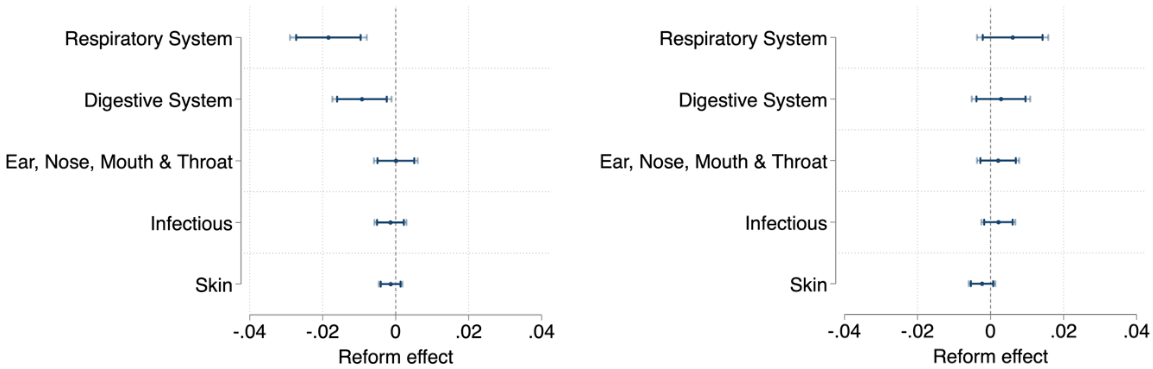
D) Effect of change to fortnightly payments



Notes: The figures show the coefficient estimates and 90% and 95% confidence intervals from difference-in-discontinuities (RD-DiD) regressions. The running variable is the child's date of birth, measured in days from the policy cutoff (1 July 2004 for the introduction of the lump-sum payment and 1 January 2009 for the change to fortnightly payments). The bandwidths used in Panels (A) and (C) are 107 days (hospital utilisation) and 103 days (CPS notifications). For Panels (B) and (D), the bandwidths are 81 days (hospital utilisation) and 88 days (CPS notifications). Observations within ±14 days (two weeks) of each cutoff are excluded. The corresponding results are reported in appendix Table A3.

Figure 2: Effects of the introduction and changes to payment design of the Baby Bonus on hospital utilisation by health diagnosis

A) Effect of introduction of lump-sum payment B) Effect of change to fortnightly payments

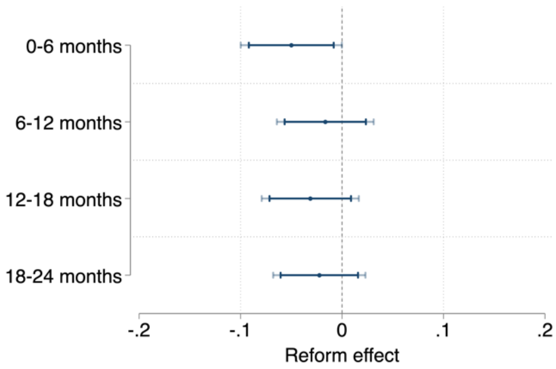


Notes: The figures show the coefficient estimates and 90% and 95% confidence intervals from difference-in-discontinuities (RD-DiD) regressions. Outcomes are measured within six months after birth. The running variable is the child's date of birth, measured in days from the policy cutoff (1 July 2004 for Panel A and 1 January 2009 for Panel B). The bandwidths used in Panels A and B are 107 and 81 days, respectively. Observations within ±14 days (two weeks) of each cutoff are excluded. The corresponding results are reported in appendix Table A4.

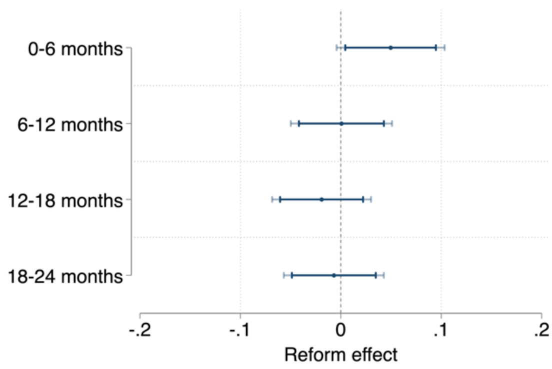
Figure 3: Effects of the introduction and changes to payment design of the Baby Bonus on the health and safety of older siblings

Any hospital utilisation

A) Effect of introduction of lump-sum payment

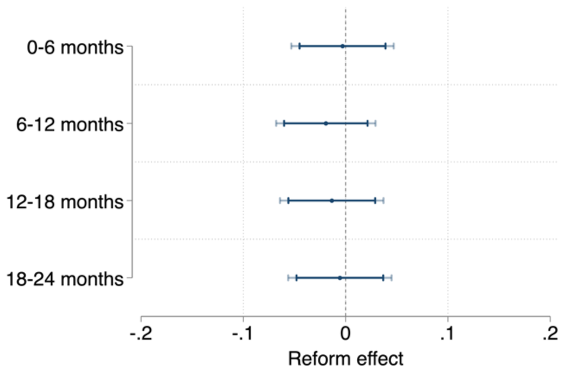


B) Effect of change to fortnightly payments

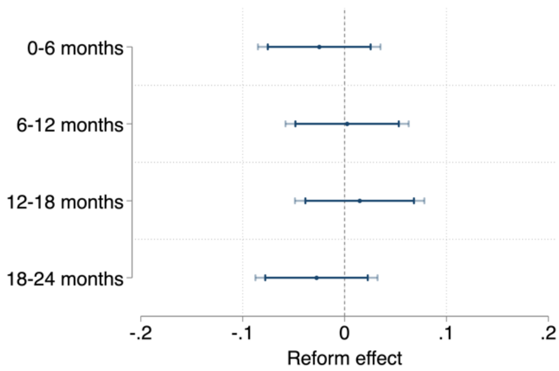


Any child protection services notification

C) Effect of introduction of lump-sum payment

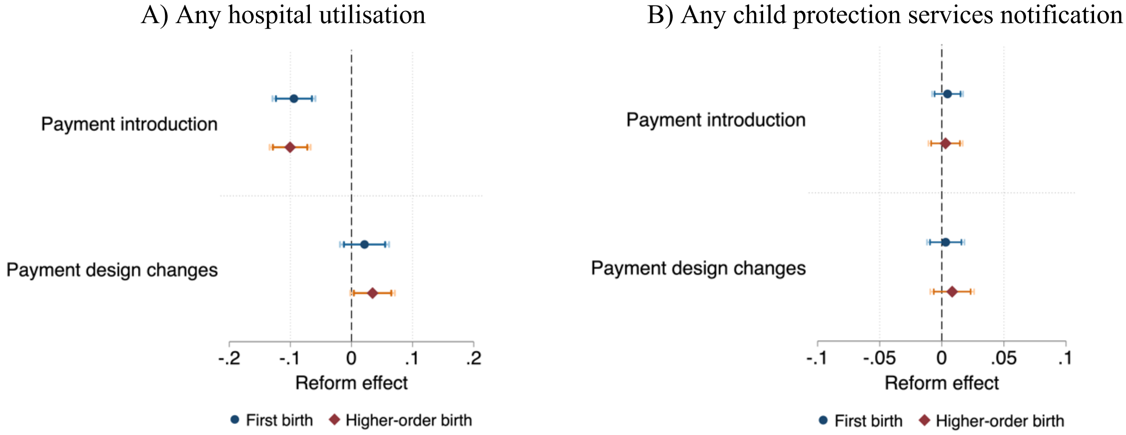


D) Effect of change to fortnightly payments



Notes: The figures show the coefficient estimates and 90% and 95% confidence intervals from difference-in-discontinuities (RD-DiD) regressions. The running variable is the child's date of birth, measured in days from the policy cutoff (1 July 2004 for the introduction of the lump-sum payment and 1 January 2009 for the change to fortnightly payments). The bandwidths used in Panels (A) and (C) are 107 days (hospital utilisation) and 103 days (CPS notifications). For Panels (B) and (D), the bandwidths are 81 days (hospital utilisation) and 88 days (CPS notifications). Observations within ±14 days (two weeks) of each cutoff are excluded. The corresponding results are reported in appendix Table A5.

Figure 4: Effects of the introduction and changes to payment design of the Baby Bonus on the health and safety of firstborn vs. later-born births



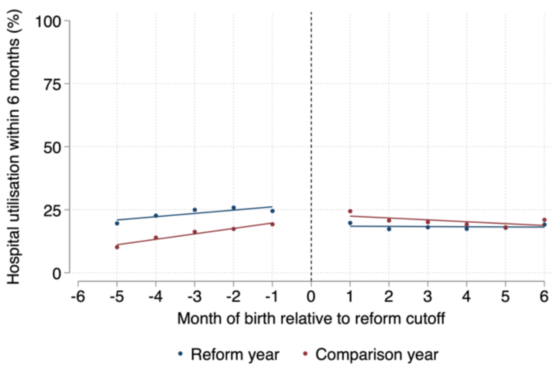
Notes: The figures show the coefficient estimates and 90% and 95% confidence intervals from difference-in-discontinuities (RD-DiD) regressions. The running variable is the child's date of birth, measured in days from the policy cutoff (1 July 2004 for the introduction of the lump-sum payment and 1 January 2009 for the change to fortnightly payments). The bandwidths used in Panel A are 107 days (payment introduction) and 81 days (payment design changes). For Panel B, the bandwidths are 103 days (payment introduction) and 88 days (payment design changes). Observations within ±14 days (two weeks) of each cutoff are excluded. The corresponding results are reported in appendix Table A6.

Supplementary appendix

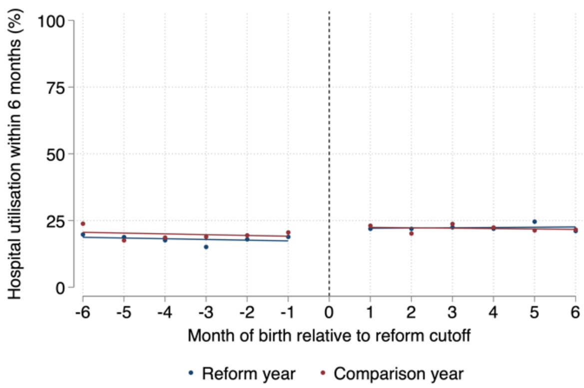
Figure A1: Average child health and safety before and after the Baby Bonus reforms in the reform year and year prior

Any hospital utilisation

A) Effect of introduction of lump-sum payment

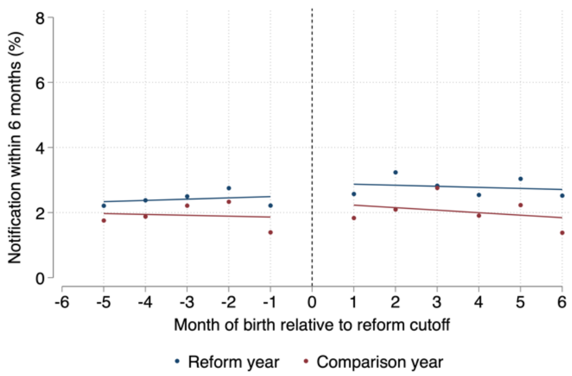


B) Effect of change to fortnightly payments

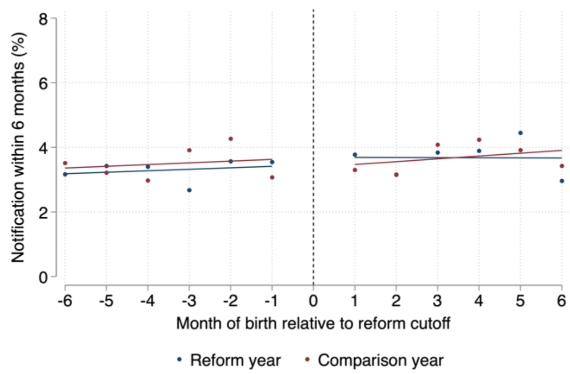


Any child protection services notification

C) Effect of introduction of lump-sum payment



D) Effect of change to fortnightly payments

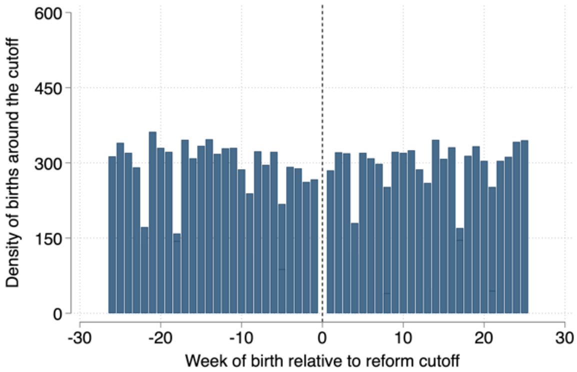


Notes: The figures plot the average share of children who were admitted to hospital (Panels A and B) and received a Child Protection Services (CPS) notification (Panels C and D) within six months after birth, by month of birth relative to the reform cutoff (1 July 2004 for the introduction of the lump-sum payment and 1 January 2009 for the change to fortnightly payments). Also shown are the equivalent plots for the year prior to the reform year (comparison year). Dots represent monthly averages, and solid lines show fitted values from local linear regressions estimated separately on each side of the cutoff.

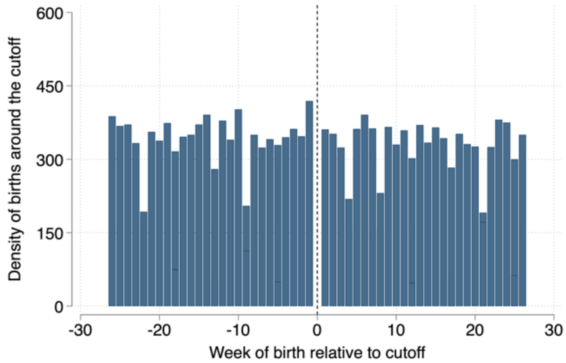
Figure A2: Distribution of the running variables (day of birth)

Reform year

A) Introduction of lump-sum payment

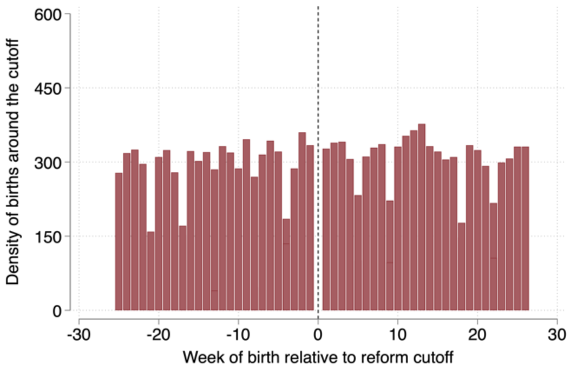


B) Change to fortnightly payments

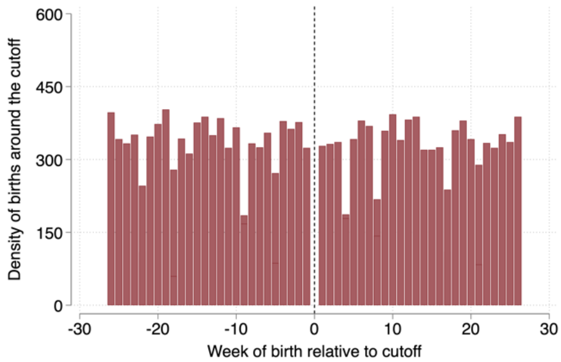


Comparison year

C) Introduction of lump-sum payment



D) Change to fortnightly payments

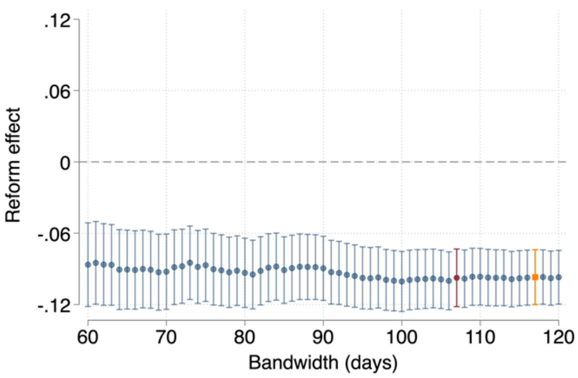


Notes: The figures show the distribution of the running variable (day of birth) for the reform years and comparison years (year prior to reform). The cut-off dates for reform years are 1 July 2004 for the introduction of the lump-sum payment and 1 January 2009 for the change to fortnightly payments. The corresponding cut-off dates for the comparison years are 1 July 2003 and 1 January 2008 respectively.

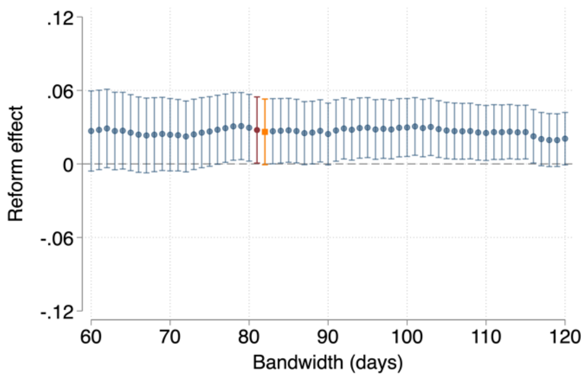
Figure A3: Robustness to alternative bandwidth choices

Any hospital utilisation

A) Effect of introduction of lump-sum payment

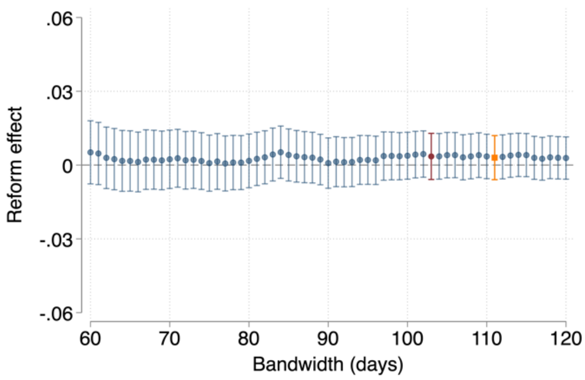


B) Effect of change to fortnightly payments

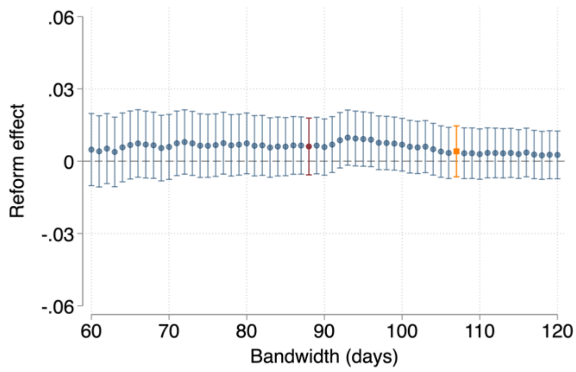


Any child protection services notification

C) Effect of introduction of lump-sum payment



D) Effect of change to fortnightly payments

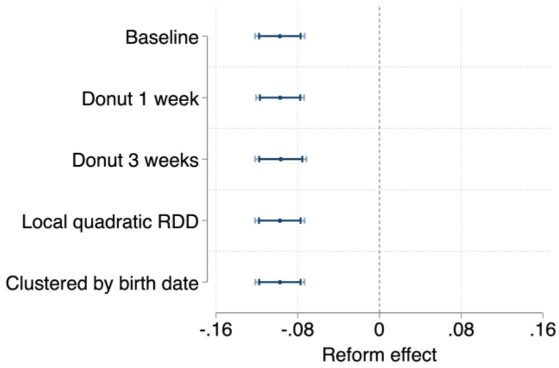


Notes: Each panel presents robustness checks using different bandwidths around the policy cutoff for the 2004 introduction of the lump-sum payment and the 2009 change to fortnightly payments. The figures plot the estimated effects of the reform on the probability of receiving a hospital utilisation and child protection services (CPS) notification within six months after birth, with 95% confidence intervals. The red marker indicates the optimal bandwidth used in the main RD-DiD analysis reported in Figure 1. The orange marker indicates the bandwidth calculated using the optimal bandwidth of Kettlewell and Siminski (2022).

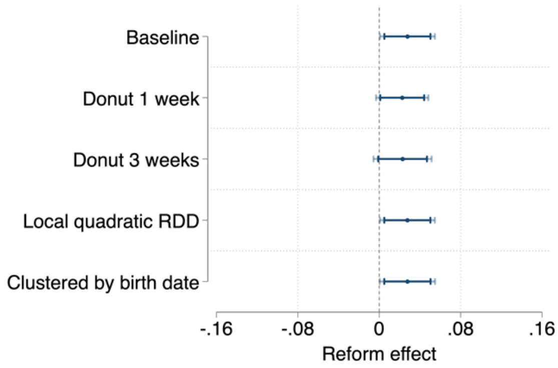
Figure A4: Effects of the introduction and changes to payment design of the Baby Bonus on child health and safety – alternative specifications

Any hospital utilisation

A) Effect of introduction of lump-sum payment

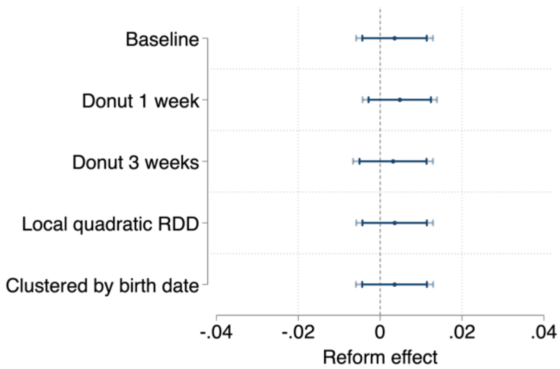


B) Effect of change to fortnightly payments

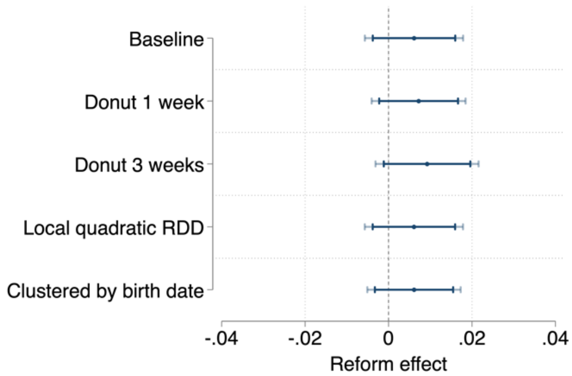


Any child protection services notification

C) Effect of introduction of lump-sum payment



D) Effect of change to fortnightly payments

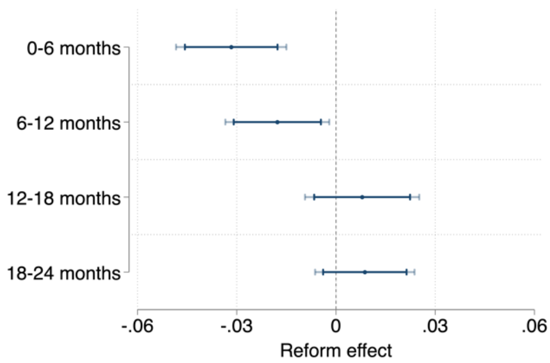


Notes: The figures show the coefficient estimates and 90% and 95% confidence intervals from difference-in-discontinuities (RD-DiD) regressions. The running variable is the child's date of birth, measured in days from the policy cutoff (1 July 2004 for the introduction of the lump-sum payment and 1 January 2009 for the change to fortnightly payments). The bandwidths used in Panels (A) and (C) are 81 days (hospital utilisation) and 88 days (CPS notifications). For Panels (B) and (D), the bandwidths are 107 days (hospital utilisation) and 103 days (CPS notifications). Observations within ±14 days (two weeks) of each cutoff are excluded.

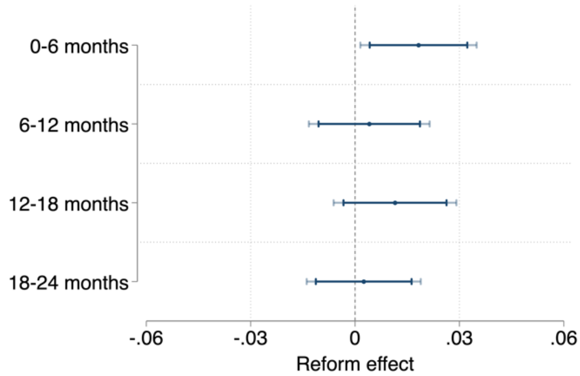
Figure A5: Effects of the introduction and changes to payment design of the Baby Bonus on child health by type of hospital utilisation

Any non-emergency hospital admission

A) Effect of introduction of lump-sum payment

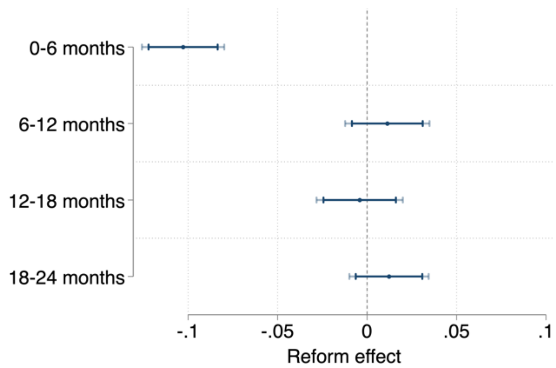


B) Effect of change to fortnightly payments

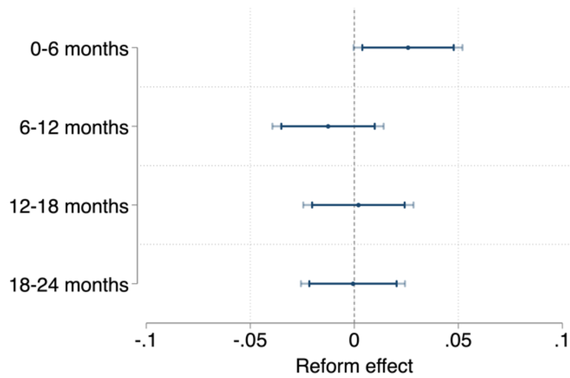


Any emergency department admission

C) Effect of introduction of lump-sum payment



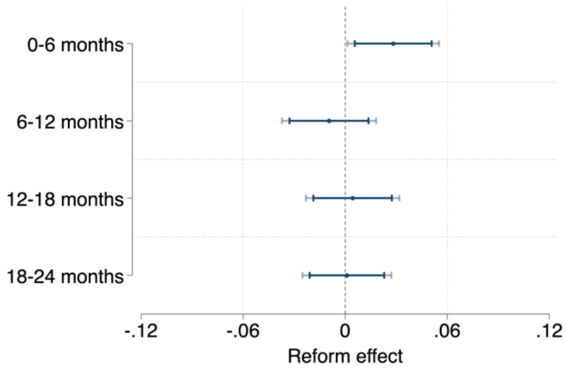
D) Effect of change to fortnightly payments



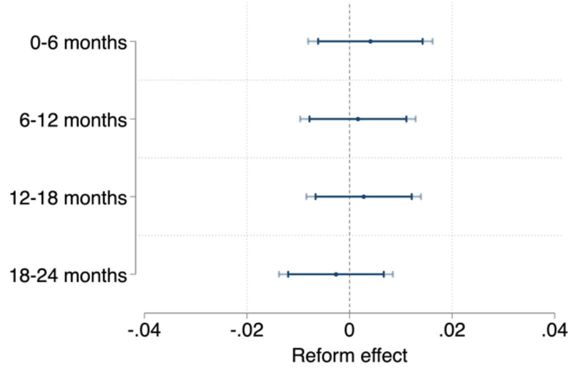
Notes: The figures show the coefficient estimates and 90% and 95% confidence intervals from difference-in-discontinuities (RD-DiD) regressions. The running variable is the child's date of birth, measured in days from the policy cutoff (1 July 2004 for the introduction of the lump-sum payment and 1 January 2009 for the change to fortnightly payments). The bandwidths used in Panels A and C are 107 days. The bandwidths used in Panels B and D are 81 days, respectively. Observations within ±14 days (two weeks) of each cutoff are excluded.

Figure A6: Effects of the changes to payment design of the Baby Bonus on child health and safety – including mothers younger than 18

A) Any hospital utilisation



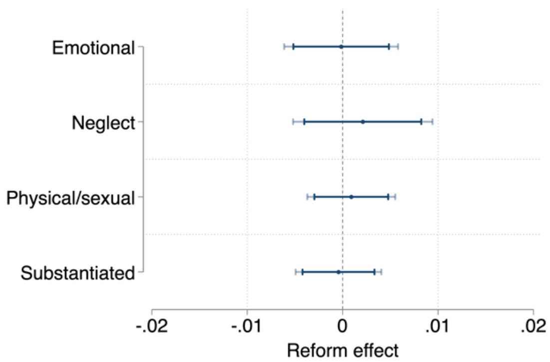
B) Any child protection services notification



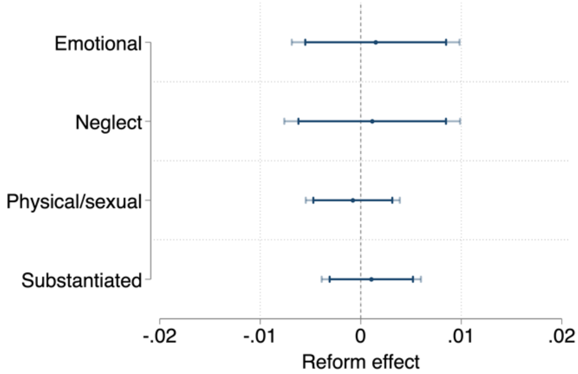
Notes: The figures show the coefficient estimates and 90% and 95% confidence intervals from difference-in-discontinuities (RD-DiD) regressions. The running variable is the child's date of birth, measured in days from the policy cutoff (1 January 2009). The bandwidths are 81 days (hospital utilisation) and 88 days (CPS notifications). Observations within ±14 days (two weeks) of each cutoff are excluded.

Figure A7: Effects of the introduction and changes to payment design of the Baby Bonus on child safety by type of maltreatment

A) Effect of introduction of lump-sum payment



B) Effect of change to fortnightly payments



Notes: The figures show the coefficient estimates and 90% and 95% confidence intervals from difference-in-discontinuities (RD-DiD) regressions. Outcomes are measured within six months after birth. Emotional abuse, Neglect and Physical/Sexual abuse refer to the type of maltreatment recorded by child protection services. Substantiated indicates a formal determination that the maltreatment occurred following an investigation. The running variable is the child's date of birth, measured in days from the policy cutoff (1 July 2004 for the introduction of the lump-sum payment and 1 January 2009 for the change to fortnightly payments). The bandwidths used in Panels A and B are 103 and 88 days, respectively. Observations within ±14 days (two weeks) of each cutoff are excluded.

Table A1: Timeline of Baby Bonus

Date	Changes
1 July 2004	First Child Tax Refund replaced by an A$3,000 tax-free payment, initially called the Maternity Payment. Lump-sum payment, not means-tested (announced 11 May 2004)
1 July 2006	Baby Bonus increased from A$3,000 to A$4,000 per child.
1 July 2007	Mothers under 18 began receiving payments in 13 fortnightly instalments.
1 July 2008	Payment increased to A$5,000 per child.
1 January 2009	Became means-tested (income limit: A$75,000) and paid in 13 fortnightly instalments to all parents.
1 March 2013	Payment amount reduced: A$5,000 for first child, A$3,000 for subsequent children.
1 March 2014	Baby Bonus abolished.

Table A2: Covariate balance

	Female	Low SES	Single mom	First birth
	(1)	(2)	(3)	(4)
Panel A: Payment introduction				
Reform effect	0.021	-0.005	0.019*	0.017
	(0.016)	(0.016)	(0.010)	(0.016)
Observations	16,322	16,322	16,295	16,320
Pre-treatment mean	0.488	0.494	0.126	0.470
Bandwidth (days)	103	103	103	103
Panel B: Change to fortnightly payments				
Reform effect	-0.026	0.007	0.007	-0.011
	(0.016)	(0.016)	(0.010)	(0.016)
Observations	15,063	15,063	15,055	15,061
Pre-treatment mean	0.489	0.533	0.106	0.465
Bandwidth (days)	88	88	88	88

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses. All specifications are estimated using difference-in-discontinuities (RD-DiD) regressions. Outcomes are measured at birth. The running variable is the child's date of birth, measured in days from the policy cutoff (1 July 2004 for Panel A and 1 January 2009 for Panel B). Observations within ±14 days (two weeks) of each cutoff are excluded.

Table A3: Effects of the introduction and changes to payment design of the Baby Bonus on child health and safety

	Hospital utilisation after birth				Child protection services notifications after birth			
	0–6 months	6–12 months	12–18 months	18–24 months	0–6 months	6–12 months	12–18 months	18–24 months
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Payment introduction								
Reform effect	-0.098***	0.011	-0.008	0.011	0.004	0.001	-0.001	0.003
	(0.012)	(0.013)	(0.013)	(0.012)	(0.005)	(0.004)	(0.005)	(0.005)
Observations	17,123	17,123	17,123	17,123	16,322	16,322	16,322	16,322
Mean dep. var. (pre-reform)	0.221	0.194	0.241	0.176	0.022	0.020	0.023	0.026
Bandwidth (days)	107	107	107	107	103	103	103	103
Effect size (%)	-44.187	5.438	-3.380	6.034	15.985	6.867	-2.354	10.984
Panel B: Change to fortnightly payments								
Reform effect	0.028**	-0.012	0.004	-0.000	0.006	0.003	0.004	-0.001
	(0.014)	(0.014)	(0.014)	(0.013)	(0.006)	(0.006)	(0.006)	(0.005)
Observations	13,605	13,605	13,605	13,605	15,062	15,062	15,062	15,062
Mean dep. var. (pre-reform)	0.184	0.232	0.199	0.199	0.035	0.028	0.029	0.028
Bandwidth (days)	81	81	81	81	88	88	88	88
Effect size (%)	15.024	-5.085	1.950	-0.118	17.334	12.372	14.641	-5.060

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Robust standard errors in parentheses. Columns (1)-(4) use a binary outcome for hospital utilisation. Columns (5)-(8) report estimates for a binary outcome indicating whether the child received a Child Protection Services (CPS) notification. All specifications are estimated using difference-in-discontinuities (RD-DiD) regressions. The running variable is the child's date of birth, measured in days from the policy cutoff (1 July 2004 for Panel A and 1 January 2009 for Panel B). Observations within ±14 days (two weeks) of each cutoff are excluded.

Table A4: Effects of the introduction and changes to payment design of the Baby Bonus on child health by hospital diagnosis

	Respiratory System	Digestive System	Ear, Nose, Mouth & Throat	Infectious	Skin
	(1)	(2)	(3)	(4)	(5)
Panel A: Payment introduction					
Reform effect	-0.018***	-0.009**	0.000	-0.001	-0.001
	(0.005)	(0.004)	(0.003)	(0.002)	(0.002)
Observations	17,123	17,123	17,123	17,123	17,123
Mean dep. var. (pre-reform)	0.046	0.020	0.012	0.005	0.002
Bandwidth (days)	107	107	107	107	107
Effect size (%)	-39.699	-45.465	0.403	-29.916	-56.007
Panel B: Change to fortnightly payments					
Reform effect	0.006	0.003	0.002	0.002	-0.002
	(0.005)	(0.004)	(0.003)	(0.002)	(0.002)
Observations	13,605	13,605	13,605	13,605	13,605
Mean dep. var. (pre-reform)	0.011	0.016	0.006	0.006	0.002
Bandwidth (days)	81	81	81	81	81
Effect size (%)	54.161	17.391	33.845	38.979	-97.782

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses. All specifications are estimated using difference-in-discontinuities (RD-DiD) regressions. Outcomes are measured within six months after birth. The running variable is the child's date of birth, measured in days from the policy cutoff (1 July 2004 for Panel A and 1 January 2009 for Panel B). Observations within ±14 days (two weeks) of each cutoff are excluded.

Table A5: Effects of the introduction and changes to payment design of the Baby Bonus on health and safety of older siblings

	Hospital utilisation after birth				Child protection services notifications after birth			
	0–6 months	6–12 months	12–18 months	18–24 months	0–6 months	6–12 months	12–18 months	18–24 months
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Payment introduction								
Reform effect	-0.050**	-0.017	-0.031	-0.022	-0.003	-0.019	-0.014	-0.006
	(0.025)	(0.024)	(0.024)	(0.023)	(0.026)	(0.025)	(0.026)	(0.026)
Observations	4,766	4,766	4,766	4,766	2,603	2,603	2,603	2,603
Mean dep. var. (pre-reform)	0.247	0.224	0.232	0.194	0.104	0.101	0.120	0.124
Bandwidth (days)	107	107	107	107	103	103	103	103
Effect size (%)	-20.240	-7.387	-13.508	-11.570	-2.928	-19.079	-11.285	-4.553
Panel B: Change to fortnightly payments								
Reform effect	0.050*	0.001	-0.019	-0.007	-0.025	0.002	0.015	-0.028
	(0.027)	(0.026)	(0.025)	(0.025)	(0.031)	(0.031)	(0.032)	(0.031)
Observations	4,173	4,173	4,173	4,173	2,239	2,239	2,239	2,239
Mean dep. var. (pre-reform)	0.266	0.235	0.203	0.231	0.159	0.155	0.170	0.154
Bandwidth (days)	81	81	81	81	88	88	88	88
Effect size (%)	18.638	0.252	-9.452	-3.004	-15.649	1.597	8.687	-17.924

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses. Columns (1)-(4) use a binary outcome for hospital utilisation. Columns (5)-(8) report estimates for a binary outcome indicating whether the child received a Child Protection Services (CPS) notification. All specifications are estimated using difference-in-discontinuities (RD-DiD) regressions. The running variable is the child's date of birth, measured in days from the policy cutoff (1 July 2004 for Panel A and 1 January 2009 for Panel B). Observations within ±14 days (two weeks) of each cutoff are excluded.

Table A6: Effects of the introduction and changes to payment design of the Baby Bonus on child health and safety – firstborn vs. later born births

	Hospital utilisation within 6 months after birth		CPS notifications within 6 months after birth	
	First birth	Higher-order birth	First birth	Higher-order birth
	(1)	(2)	(3)	(4)
Panel A: Payment introduction				
Reform effect	-0.094***	-0.100***	0.005	0.003
	(0.018)	(0.017)	(0.006)	(0.007)
Observations	8,060	9,061	7,675	8,645
Mean dep. var. (pre-reform)	0.210	0.230	0.021	0.023
Bandwidth (days)	107	107	103	103
Effect size (%)	-44.828	-43.700	22.104	13.152
Panel B: Change to fortnightly payments				
Reform effect	0.021	0.035*	0.003	0.009
	(0.021)	(0.019)	(0.008)	(0.009)
Observations	6,299	7,305	6,995	8,065
Mean dep. var. (pre-reform)	0.197	0.173	0.025	0.044
Bandwidth (days)	81	81	88	88
Effect size (%)	10.821	19.951	12.741	18.922

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses. Columns (1)-(2) use a binary outcome for hospital utilisation. Columns (3)-(4) report estimates for a binary outcome indicating whether the child received a Child Protection Services (CPS) notification. All specifications are estimated using difference-in-discontinuities (RD-DiD) regressions. The running variable is the child's date of birth, measured in days from the policy cutoff (1 July 2004 for Panel A and 1 January 2009 for Panel B). Observations within ±14 days (two weeks) of each cutoff are excluded.

Table A7: Effects of the introduction and changes to payment design of the Baby Bonus on child health and safety – Using a comparison year two years before the reform

	Hospital utilisation within 6 months after birth	Child protection services notifications within 6 months after birth
	(1)	(2)
Panel A: Payment introduction		
Reform effect	-0.041***	0.002
	(0.011)	(0.005)
Observations	16,964	16,166
Mean dep. var. (pre-reform)	0.176	0.022
Bandwidth (days)	107	103
Effect size (%)	-23.247	10.550
Panel B: Change to fortnightly payments		
Reform effect	-0.011	0.009
	(0.014)	(0.006)
Observations	13,402	14,821
Mean dep. var. (pre-reform)	0.178	0.033
Bandwidth (days)	81	88
Effect size (%)	-6.281	26.573

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses. Column (1) uses a binary outcome for hospital utilisation. Column (2) reports estimates for a binary outcome indicating whether the child received a Child Protection Services (CPS) notification. All specifications are estimated using difference-in-discontinuities (RD-DiD) regressions. The running variable is the child's date of birth, measured in days from the policy cutoff (1 July 2004 for Panel A and 1 January 2009 for Panel B). Observations within ±14 days (two weeks) of each cutoff are excluded.

Table A8: Effects of the introduction and changes to payment design of the Baby Bonus on child health and safety – Extended observation period

	Hospital utilisation after birth		Child protection services notifications after birth	
	24–30 months	30–36 months	24–30 months	30–36 months
	(1)	(2)	(3)	(4)
Panel A: Payment introduction				
Reform effect	-0.011	0.017	-0.006	0.007
	(0.011)	(0.011)	(0.005)	(0.005)
Observations	17,123	17,123	16,322	16,322
Mean dep. var. (pre-reform)	0.176	0.135	0.025	0.024
Bandwidth (days)	107	107	103	103
Effect size (%)	-6.526	12.610	-25.124	29.311
Panel B: Change to fortnightly payments				
Reform effect	-0.014	-0.003	-0.002	0.006
	(0.012)	(0.012)	(0.006)	(0.006)
Observations	13,605	13,605	15,062	15,062
Mean dep. var. (pre-reform)	0.151	0.155	0.034	0.032
Bandwidth (days)	81	81	88	88
Effect size (%)	-8.987	-1.983	-4.958	19.965

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses. Columns (1)-(2) use a binary outcome for hospital utilisation. Columns (3)-(4) report estimates for a binary outcome indicating whether the child received a Child Protection Services (CPS) notification. All specifications are estimated using difference-in-discontinuities (RD-DiD) regressions. The running variable is the child's date of birth, measured in days from the policy cutoff (1 July 2004 for Panel A and 1 January 2009 for Panel B). Observations within ±14 days (two weeks) of each cutoff are excluded.

Table A9: Effects of the introduction and changes to payment design of the Baby Bonus on number of hospital admissions and child protection services notifications

	Number of hospital admissions within 6 months after birth	Number of child protection services notifications within 6 months after birth
	(1)	(2)
Panel A: Payment introduction		
Reform effect	-0.237***	0.005
	(0.031)	(0.009)
Observations	17,123	16,322
Mean dep. var. (pre-reform)	0.416	0.034
Bandwidth (days)	107	103
Effect size (%)	-57.049	14.482
Panel B: Change to fortnightly payments		
Reform effect	0.073**	0.006
	(0.034)	(0.006)
Observations	13,605	15,062
Mean dep. var. (pre-reform)	0.334	0.035
Bandwidth (days)	81	88
Effect size (%)	21.853	17.334

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses. Column (1) reports estimates for the outcome measuring the number of hospital utilisations. Column (2) reports estimates for the outcome measuring the number of Child Protection Services (CPS) notifications. All specifications are estimated using difference-in-discontinuities (RD-DiD) regressions. The running variable is the child's date of birth, measured in days from the policy cutoff (1 July 2004 for Panel A and 1 January 2009 for Panel B). Observations within ±14 days (two weeks) of each cutoff are excluded.

Table A10: Effects of the introduction and changes to payment design of the Baby Bonus on child health and safety – including children vulnerable at birth

	Hospital utilisation within 6 months after birth	Child protection services notifications within 6 months after birth
	(1)	(2)
Panel A: Payment introduction		
Reform effect	-0.091***	0.003
	(0.012)	(0.005)
Observations	17,969	17,137
Mean dep. var. (pre-reform)	0.230	0.024
Bandwidth (days)	107	103
Effect size (%)	-39.387	11.968
Panel B: Change to fortnightly payments		
Reform effect	0.025*	0.006
	(0.014)	(0.006)
Observations	14,316	15,847
Mean dep. var. (pre-reform)	0.193	0.038
Bandwidth (days)	81	88
Effect size (%)	12.925	16.779

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses. Column (1) reports estimates for the outcome measuring the number of hospital utilisations. Column (2) reports estimates for the outcome measuring the number of Child Protection Services (CPS) notifications. All specifications are estimated using difference-in-discontinuities (RD-DiD) regressions. The running variable is the child's date of birth, measured in days from the policy cutoff (1 July 2004 for Panel A and 1 January 2009 for Panel B). Observations within ±14 days (two weeks) of each cutoff are excluded.